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Psychiatry Versus Societal Reaction: A Critical Analysis
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Source: *Journal of Health and Social Behavior*, Vol. 21, No. 3 (Sep., 1980), pp. 268-278
Published by: American Sociological Association
Stable URL: http://www.jstor.org/stable/2136621
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Psychiatry Versus Societal Reaction: A Critical Analysis

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Journal of Health and Social Behavior 1980, Vol. 21 (September):268–278

The debate between proponents of clinical psychiatry and those who advocate a societal reaction approach to mental disorders has continued for over twenty years. This paper draws on several crucial empirical studies from research psychiatry to make the following points: (1) levels of reliability and validity in psychiatry are low compared with those in other branches of medicine; (2) they are considerably lower than psychiatry's most ardent advocates claim, but somewhat higher than its most severe critics contend; (3) the functional mental disorders present special problems because of their lack of demonstrable organic pathology and the lack of convergent but independent diagnostic indices; (4) the best empirical evidence suggests that psychiatric and social factors interact to determine case outcome and that the latter frequently outweigh the former in their effects; (5) isolating these interaction effects is complicated by the factors mentioned in (3) but can be accomplished by careful research design. Finally, some testable questions that would help to resolve the controversy are proposed.

The debate between psychiatrists, who view mental disorders as diseases, and their critics, who propose that mental disorders are deviant social roles, has continued for over twenty years (Szasz, 1961; Ausubel, 1961; Scheff, 1966, 1975; Gove, 1975). Recently Gove (1979) has claimed that the debate continues because sociologists have ignored the work of psychiatrists. I contend that this statement is only half correct. The debate has continued because *both* sides have ignored critical empirical research from psychiatry and because the debate has seldom centered on testable questions. This discussion will attempt to rectify this situation by analyzing several crucial studies that help to clarify both the validity *and* the limitations of the societal reaction approach. In so doing, I will refer to Gove's recent attack (1979) on labeling theory, because it embodies and depends upon the fundamental assumptions of clinical psychiatry: (1) mental disorders are diseases and should be treated as such; (2) psychiatric treatments are relatively effective and are seldom harmful to the patient.

Although much of my discussion could apply generally to the functional mental disorders, the research I will evaluate concentrates on the major psychoses (i.e., schizophrenia and the affective psychoses) and their treatment. The "societal reaction" approach will be defined below.

Thanks are due James R. Greenley and John S. Strauss, M.D., for their helpful comments on a previous version of this paper and Patricia J. Waite for aid in manuscript preparation.

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LIMITATIONS OF PSYCHIATRY

In defending the psychiatric approach to mental disorders, Gove (1979) maintains that, although psychiatric explanations of mental disorders are rather imprecise, they are probably at least as cogent as the better sociological explanations. This is, of course, irrelevant. Psychiatry is a branch of medicine, and if it is to maintain credibility, it must defend itself as a medical specialty. This raises the issues of reliability and validity: How reliable and valid are psychiatric diagnoses and treatment compared with those in other branches of medicine?

Reliability

In his trenchant analysis of psychiatric diagnosis, the research psychiatrist R. E. Kendell (1975) notes that since 1950, some 30 or 40 studies of reliability have been conducted, but between them they tell us little except that reliability is often very low, and generally lower for neuroses and personality disorders than for psychoses and organic states. How-

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ever, reliability can be increased by standardization: structured interviewing techniques and common training and definitions. In research settings at least, the introduction of such techniques enables "acceptable levels of concordance" to be reached with diagnoses of the traditional Kraepelinian type.

Research settings, however, are somewhat "artificial" because many possible sources of diagnostic variation are controlled. For example, many research designs call for blind raters to evaluate written summaries of psychiatric interviews rather than evaluate real patients (Kety, 1974; Turner and Gartrell, 1978). This procedure tends to reduce many potential sources of variability: different background and training of evaluators; pressure to treat or hospitalize; variability exhibited by the patients over time. Consequently, reliability in ordinary clinical settings must differ considerably.

No large-scale studies of diagnostic reliability in ordinary clinical settings have been mounted since the early sixties but, as Kendell notes (1975), ". . . evidence of the depths to which diagnostic reliability is capable of sinking has continued to accumulate" (p. 37).

Systematic comparisons of British and American psychiatrists exemplify the problem of low reliability in psychiatric diagnosis. Katz et al. (1969) found that agreement between British and American psychiatrists was quite high for patients showing the archetypal symptoms of schizophrenia or psychotic depression. Agreement was distressingly low for patients with more ambiguous symptoms. More alarming was the pronounced tendency of American psychiatrists to diagnose schizophrenia in patients who had been specifically picked to represent nonpsychotic disorders (e.g., neurosis, character disorder). A more extensive comparison of British and American psychiatrists produced similar findings (Kendell et al., 1971). Patients showing classic, textbook symptoms evoked almost unanimous agreement. But a majority (69%–85%) of the American psychiatrists diagnosed schizophrenia in patients picked to represent nonpsychotic disorders, whereas the British almost shunned this category of diagnosis (2%–7%).

Some might argue that these comparisons do not indicate a general lack of validity in psychiatric diagnosis but rather that the two nationalities merely used different "instruments" in diagnosing schizophrenia. After all, though the diagnostic variation between the nationalities was quite high, the variation within the national groups was within reasonable limits (Cooper et al., 1969). There are three arguments that militate against this interpretation. First, American psychiatrists tended to diagnose schizophrenia in patients picked specifically to represent nonpsychotic disorders. These discrepancies cannot be dismissed as mere semantic variation. Persons diagnosed as psychotic are much more likely than nonpsychotics to be (1) treated involuntarily, (2) hospitalized, (3) given phenothiazines and electroconvulsive therapy (ECT), and (4) declared mentally and legally incompetent. Diagnostic reliability, then, is not merely of academic interest; it involves weighty moral, legal, and medical considerations.

Second, the general agreement on diagnosis within the American group may reflect fairly high reliability but low validity. That is, the American psychiatrists "agreed" because they tended to diagnose almost anyone with the status of mental patient as schizophrenic. The authors of the more extensive study (Kendell et al., 1971) comment on this possibility at length:

> Although . . . no concept of schizophrenia can be either right or wrong, it does seem, at least to a European observer, that the diagnosis is now made so freely on the east coast of the United States that it is losing much of its original meaning and is approaching the point at which it becomes a synonym for functional mental illness. Seven of the eight patients in this study were diagnosed as schizophrenic by over two thirds of the American psychiatrists, although between them they presented a variety of different symptoms and problems. Similarly, in a recent random sample of 192 patients below the age of 60 admitted to public mental hospitals in New York City, 82% of those with nonorganic conditions were diagnosed as schizophrenic by the hospital psychiatrists. (p. 129)

Third, the British-American comparisons are consistent with studies suggesting that American psychiatrists can perceive relatively serious psychopathology almost anywhere. Temerlin (1968), for example, showed that the

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mere suggestion by a prestigious colleague that the subject was psychotic could induce 60% of a group of psychiatrists to diagnose as psychotic an actor portraying the ideal, normal man. The actor's "ideal, normal" responses to interview questions had been prepared by a group of clinicians. In contrast, control subjects heard a prestigious colleague remark that the man was perfectly normal; none of the control subjects diagnosed him as psychotic. Similarly, Rosenhan (1973) showed that researchers who feigned psychotic symptoms to gain entrance to mental hospitals were not differentiated from real patients by staff members—even after they stopped simulating symptoms. (Interestingly, some of the real patients in the hospitals did discover the ruse.) In screening a rural community, Beiser (1971) found that interviewing psychiatrists had trouble recognizing anybody as normal. In fact, the team was never able to find enough individuals for their research design who fit the category "almost certainly not a psychiatric case," and the subjects from the next category, "doubtful (but probably not a case)," had to be substituted. These and other studies suggest that results which show relatively high levels of diagnostic reliability among American clinicians may reflect a *lack* of scientific validity rather than its presence. It was this possibility that the investigators in the British-American study (Kendell et al., 1971) found so alarming:

> But disagreements as glaring as this have serious implications. Diagnoses are the most important of all our technical terms because they are the means by which we identify the subject matter of most of our research. They identify the types of patients who received the drug we were assessing, or whose family dynamics or sodium metabolism we were studying. If these terms are used by different groups of psychiatrists in widely differing ways, the two will, at best, fail to communicate with each other, and may well actively mislead one another. (p. 127)

In short, without reliable classification, a science of psychiatry becomes impossible (Townsend, 1978, 1979; Kendell, forthcoming).

The results of a highly sophisticated statistical analysis of diagnostic validity and reliability by Strauss et al. (1979) are consistent with the British-American comparisons. This analysis shows that although archetypal psychiatric symptoms do exist in patient populations, the great majority of symptoms are much less severe and distinctive. Furthermore, in contrast to symptoms, the great majority of first admission patients *can not* be grouped according to the basic syndromes (and ambulatory patients even less so). As the authors remark:

> The implications of these findings for clinical practice and research are considerable. The diagnostician dealing with real patients is apparently forced to place patients into discrete diagnostic categories based on relatively severe levels of symptomatology, even though a great number of patients actually fall between the categories or exhibit low symptom levels *where the categories themselves have relatively little meaning* [emphasis supplied].
>
> . . . Both clinician and investigator are led to categorize these patients in ways that simply do not fit the actual nature of the population. (p. 111)

The fact that a majority of patients show low levels of ambiguous symptoms serves to deflate levels of diagnostic reliability. Logically, then, it is these patients that best support a fundamental tenet of the societal reaction school: Social factors are often better predictors of psychiatric decisions than the patient's clinical status. The effects of these factors, however, are seldom formally acknowledged in psychiatry.

These studies do not prove that there are no psychotics, nor do they show that no patients are reliably diagnosable. Rather, they demonstrate that under certain conditions almost anybody can be perceived as psychotic (at least in the United States). Moreover, they point to the lack of independent validating criteria in psychiatric diagnoses. In other medical specialties, accurate diagnosis depends on the principle of convergent but independent diagnostic indices: lab tests, the patient's somatic complaints, pathology reports, tissue committees, EKG, X rays. In psychiatry this principle is virtually nonexistent. Psychiatric diagnosis always depends on the clinician's impressions of the patient's behavior. This means that psychiatric diagnoses are not objectively falsifiable or reversible (Neisser, 1973). In short, the lack of demonstrable organic pathology and independent diagnostic indices are crucial determinants of the rela-

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tively low levels of reliability and validity in psychiatry. More detailed investigations of these levels and their effects seem in order.

Validity

As Kendell notes (1975:39–41), reliability is just a means to an end. It is important because it establishes a ceiling for validity. The lower reliability becomes, the lower validity sinks. Kendell describes four different types of validity: concurrent, predictive, construct, and content. Because of the applied nature of clinical intervention, Kendell regards (rightly, I think) predictive validity as by far the most important. I will therefore concentrate my discussion on it.

Kendell (1975:41) observes that few studies of predictive validity have been mounted in psychiatry because much of the evidence for the Kraepelinian categories accumulated before anyone began to ask questions about reliability and predictability. In other words, many of the "facts" of psychiatric diagnosis and prognosis are really *assumptions* accepted on the strength of clinical tradition rather than scientific validation. In this regard, Kendell (1975:41) remarks that two of the traditional prognoses (viz., the high suicide and relapse rates of manic-depressive illness and the chronic and degenerative nature of schizophrenia) are too well established, at least in the minds of psychiatrists, to make a test worthwhile. Fortunately, some researchers have thought otherwise.

The results of a recent (and highly sophisticated) study of outcome directly contradict the prevailing psychiatric view of schizophrenia. In a study of 142 patients, Strauss and Carpenter (1972) found that their results did not support the view that schizophrenia has a predictable outcome. Areas of outcome dysfunction (work, symptoms, social relations, and nonhospitalization) were found to operate as open systems; that is, they were partially interrelated, but each was also affected by variables specific to it alone. The authors also found that outcome for schizophrenic patients overlapped with outcome for other diagnoses (i.e., the areas of outcome dysfunction were relatively independent of diagnosis). This was true even when Kurt Schneider's (1950) narrow criteria for schizophrenia were utilized to classify patients. The authors concluded that their findings challenge the utility of outcome as a validating criterion for the concept of schizophrenia or as a criterion for differentiating schizophrenic patients from patients hospitalized with other severe functional mental disorders. In sum, the findings challenge the notion that schizophrenia (or any diagnosis) has a predictable outcome. Instead, outcome seemed to be determined by an interaction of the four areas of dysfunction: work, symptoms, social relations, and length of hospitalization.

Apparently, if prediction of outcome is used as a measure, then the predictive validity of psychiatric diagnosis is quite low. Another measure of validity is the ability to predict treatment response. If one can predict the different responses to treatment of different diagnostic groups, then the predictive validity of these diagnostic categories is enhanced. This measure of validity, however, is no more reassuring than the prediction of outcome. To be sure, certain clinical features tend to correlate with a particular response to treatment (e.g., the effects of ECT on the symptoms of manic-depressive depression). But as Kendell notes (1975):

> The fact that each of the treatments discussed above is more effective with one category than with others tends to establish the validity of these categories, but the fact that each is effective to some extent in several categories, and inconsistent within all of them, does the reverse. (p. 43)

Indeed, in his most recent analysis of diagnosis, Kendell (forthcoming) acknowledges that the "cardinal failing" of psychiatric diagnoses is their inability, compared with those in other branches of medicine, to predict outcome and treatment response.

There is an even stronger reason for not trusting treatment response as a measure of predictive validity: Frequently little is known about the negative responses to these treatments. Recent reports in the *American Journal of Psychiatry* (Asnis et al., 1977; Carpenter et al., 1977) point out that the present practice of routine administration of massive doses of neuroleptics to mental patients is based on untested and unwarranted assumptions. These drugs have serious, harmful, and irreversible

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effects for some patients, but it is presently impossible to predict which patients are vulnerable (Asnis et al., 1977). Consequently, most patients should at least be given a trial period without drugs, but most patients do not receive such a trial. The authors of one of these reports (Carpenter et al., 1977) conclude that current treatment attitudes in psychiatry far outdistance their informational base. There is similar controversy concerning the efficacy and possible harmful effects of electroconvulsive therapy (Dongier and Wittkower, forthcoming).

Gove's claim (1979:302) that psychiatric treatment (control) of serious symptoms is "very effective" must be viewed in the light of the preceding studies. If by "treatment (control)" Gove means the effective suppression of disruptive behavior by possibly dangerous means, his claim is certainly accurate. If, however, he means diagnoses, prognoses, and treatment modes on which there are generally high levels of agreement, these are much more problematic in psychiatry than in other branches of medicine (Kendell, 1975, forthcoming).

The lack of demonstrable organic pathology and independent diagnostic indices in psychiatry has important ramifications for a sociocultural approach to mental disorders. In this regard Gove (1979:303) has complained that social scientists recognize the interaction of "social" and "medical" factors in the treatment of physical disorders, but in the study of mental disorders interpret the importance of social factors as proof of the invalidity of psychiatry. Actually, the social scientist's reaction is understandable (albeit somewhat disproportionate and obdurate). It is much more difficult to study the interaction of "social" and "medical" factors in psychiatry because it is so difficult to establish objectively what is "medical." Put another way, the relative difficulty (if not impossibility) of objectively verifying the clinical status of psychiatric patients makes it equally difficult to demonstrate that social factors rather than clinical symptoms are the primary determinants of psychiatric decisions.

The only way to tease out the influence of social factors is to institute stringent controls for severity of symptoms. When this is done, it becomes evident that social factors are important determinants of psychiatric decisions and often outweigh severity of symptoms in their effects. These effects, however, are seldom if ever acknowledged in the official rationalizations of psychiatric decisions. For example, Mendel and Rapport (1969) showed that a history of previous hospitalization (not severity of symptoms, as the decision-makers believed) was the primary determinant of the decision to hospitalize. Family desires concerning the patient's status frequently outweigh symptoms in their effects on discharge and placement decisions (Greenley, 1972, forthcoming; Steadman and Cocozza, 1974:135). Cohen et al. (1975) showed that the discharge diagnosis had changed from the intake diagnosis for 15% of their sample. The patient's occupational status seemed to be a primary determinant of these decisions.

Cocozza and Steadman (1978) have shown that psychiatrists are no more capable than laypersons of predicting the future dangerousness of mental patients, but even psychiatrists who admit this continue to make such recommendations. Furthermore, the major determinant of their recommendation is the seriousness of the alleged offense (as it is for the layperson), but this factor is seldom formally acknowledged. In discussing the relative importance of social versus clinical factors, Cocozza and Steadman (1978) document that clinical rationales

> serve not only to hide the importance of non-psychiatric factors and the impact of non-psychiatrists in the decision making process, but also to foster the illusion of the existence of a special psychiatric expertise in such decisions. (p. 274)

Limitations of Societal Reaction Theory

Having examined some of the limitations of psychiatry, I will now analyze some of the limitations of the societal reaction approach. One of the knottiest problems for this approach is the apparent cross-cultural universality of certain symptoms. Insofar as mental disorders are deviant social roles, their "symptoms" should vary with cultural role expectations. This means that intercultural variation in symptoms should greatly exceed intracultural variation. Scheff has stated this explicitly

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(1966:82–83). Present evidence, however, suggests that this is not completely the case.

Universality

It seems clear that the preceding studies of diagnosis do not justify the more grandiose claims (e.g., Murphy, 1976) for a universality of symptoms and a universal ease in recognizing them (cf. Sarbin and Mancuso, forthcoming). Nevertheless, given some variation in symptom content, and some possible variation in frequency, it does appear that conditions resembling the major psychoses in the Western world are recognized universally (Yap, 1951; Schooler and Caudill, 1964; Edgerton, 1966; Eaton and Weil, 1967; Draguns and Phillips, 1972; Kiev, 1972; Murphy, 1976). Moreover, it is also true that some of these conditions appear to follow genetic lines.

An extensive study of adoptees and their relatives in Denmark showed that the rates for schizophrenia were significantly higher among biological relatives of schizophrenics than among their adopted relatives (Kety et al., 1971; Rosenthal et al., 1971; Wender et al., 1971). This study can be criticized on a variety of grounds, including the fact that a very broad spectrum of disorders was included in the index group: symptoms of chronic, acute, and borderline schizophrenia (*as these terms are commonly used in the U.S.*). Because the criteria for inclusion were not as restricted or as specific as in some studies of diagnosis (e.g., Strauss and Carpenter, 1972; Strauss et al., 1979), it is impossible to define the implications of these findings for diagnostic reliability and validity. For example, the Denmark study showed that the "acute schizophrenia" group did not seem to follow genetic lines, whereas the "chronic" and "borderline" groups did (Kety et al., 1971). This raises some interesting questions: Are the chronic and borderline groups more reliably diagnosable than the acute group? Does diagnostic reliability for a group correlate with apparent genetic transmission? Only further research can answer these questions.

The possible flaws of this genetic research notwithstanding, societal reaction theorists are still faced with the fact that blind raters found significantly more schizophrenia among the biological relatives of the index group than among their adopted relatives; this was not true of a carefully matched group of controls. It is difficult to see how purely sociological or societal reaction approaches could explain these findings. On the other hand, the rates in the index group were not high enough to suggest a simple monogenic or an "all or none" effect. Rather, the tendency to become schizophrenic seems to be transmitted as a vulnerability or predisposition that can be evoked by environmental influences (Kety et al., 1971:304). Indeed, as Dohrenwend has remarked (1975), it is ironic that the strongest (though indirect) support for psychosocial factors in the etiology of mental disorders comes from biogenetic studies: Inheritance cannot explain all the variation; consequently, environmental factors (possibly psychosocial) must also be involved (Akiskal and McKinney, 1973).

In view of these facts, it seems clear that any comprehensive theory of mental disorders must ultimately confront the facts that there are people who spontaneously exhibit archetypal symptoms, and these initial symptoms cannot be understood as a "social role" in any conventional meaning of the term. Indeed, it is the very unpredictability of these behaviors that helps to confer the label of madness (Edgerton, 1966, 1969; Bowers, 1974; Townsend, 1979). After recognition and labeling have occurred and during the treatment phase, the concept of social role seems to become more applicable, as the patient begins to adopt a "patient role" and may even develop a quite recognizable institutional syndrome (Wing, 1962; Townsend, 1978:108–15).

Another problem for societal reaction theory is the range of reliability and validity levels in psychiatry. The levels are *generally* low compared with the levels in other branches of medicine, but for a certain percentage of the patient population they are acceptable. A minority of patients do exhibit archetypal symptoms that are reliably diagnosable, and some patients do respond favorably to psychiatric treatments. The problem is, of course, predicting which patients will respond this way (Kendell, 1975, forthcoming; Asnis et al., 1977). As Carpenter et al. (1977) point out, although current treatment attitudes in psychiatry are not totally invalid, they do "far outdistance their informational base." They are

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frequently based on unverified assumptions and thus are considerably less reliable and valid than those in other branches of medicine (Kendell, 1975, forthcoming). The work by these research psychiatrists thus suggests that psychiatric diagnosis and treatment are neither as reliable and valid as their most ardent advocates claim (e.g., Murphy, 1976; Gove, 1975, 1979), nor as invalid as their most severe critics contend. The truth lies somewhere between these polarities.

Societal Reaction vs. Social Determinants

Gove (1979:301) claims that the labeling explanation of mental illness has become untenable and is now of historical interest only. The validity of his claim depends on how one defines "labeling explanation." For this discussion societal reaction can be defined as that approach which attempts to locate the causes of continuing (careers of) deviance in society's reactions to initial deviance (Scheff, 1974, 1975, 1979). Labeling explanations, then, appear to be a subtype of societal reaction theory that focuses on the label society affixes to the deviant behavior and the effects of this label on the deviants and on those reacting to them. Scheff's analysis (1966) is probably the best-known formulation of labeling theory in the study of mental disorders. When they are defined in this way, it seems clear that the societal reaction and labeling perspectives have been applied with varying degrees of success.

In appraising the overall contributions of the societal reaction approach, the evidence seems overwhelming that certain societal reactions have produced considerable "secondary deviance." Among these reactions we would have to include psychiatric treatments, long-term hospitalization, and treatment with neuroleptics. In some cases this "secondary" deviance becomes a primary determinant of outcome. Much of the evidence supporting this proposition has come from research psychiatry (Wing, 1962; Strauss and Carpenter, 1972; Mattes et al., 1977; Asnis et al., 1977).

The success of more specific applications of the societal reaction approach has varied. Some hypotheses have been supported and some have not. For example, two major propositions of Scheff's thesis (1966)—viz., popular stereotypes of insanity provide the guidelines for symptom formation, and significant others' expectations concerning symptoms are primary determinants of symptomatology—have not been supported empirically (Townsend, 1978; Greenley, 1979). On the other hand, work by the same authors has supported more general tenets of a sociological approach to mental disorders: Sociocultural factors like family desires, cultural conceptions, and institutional requirements can be primary determinants of case outcome (Greenley, 1972, forthcoming; Townsend, 1978). The solidarity of the societal reaction approach is thus not unitary. An objective evaluation would have to show that certain planks have proven stronger than others.

Social Determinants

Societal reaction explanations of mental illness should be differentiated from more general sociocultural explanations. The former focus on societal reactions to deviance as causes of continuing deviance. The latter can presumably refer to any social factor that affects the occurrence of deviance—initial or continuing. Few would deny that social factors have such effect.

An Attempt at Integration

Having examined some of the strengths and weaknesses of both psychiatry and societal reaction theory, we must now ask ourselves how might the conflict between them be resolved. Gove suggests (1979:303) that an "interactional" view is appropriate to the task of integrating psychiatric and sociological approaches to the study of mental disorders. This suggestion has merit as far as it goes, for probably everyone would agree that social and medical factors "interact" in producing illness behavior. As noted previously, however, psychiatry presents special problems in this regard because there are presently no objective means for isolating what is "medical" (for the functional mental disorders). Consequently, conventional psychiatrists and their defenders have tended to regard as "medical" and "symptomatic" much of what some social sci-

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entists have viewed as responses to social environments. "Interactional perspective" thus means very different things to different groups. In the remaining paragraphs I will describe a specific interactional view that is supported by empirical studies from a variety of disciplines, including sociology and research psychiatry.[1] In brief, I propose, as have a number of authors previously, that social factors and societal reactions are always involved in the determination of outcome, but that the impact of their effects correlates inversely with severity of symptoms: The more disruptive or bizarre the deviant's behavior, the less noticeable the effects of social factors become (Miller, 1965, 1967; Edgerton, 1969; Rushing and Esco, 1977; Rushing, 1978).

The studies of diagnosis reviewed above suggest that a minority of patients show classic, textbook symptoms; diagnostic agreement is acceptably high for these patients. This finding is extremely compatible with empirical studies of outcome. In an extensive retrospective analysis, Miller (1965, 1967) found that symptoms interacted with social factors to determine outcome, and that a curvilinear relationship existed between severity of symptoms and desires of family members. Certain mildly distressing behaviors were tolerated by all, but radically deviant behaviors were nearly always associated with hospitalization. For middle-range behaviors, outcome varied with tolerance of deviance: If significant others were perturbed, hospitalization occurred; if they were not, community tenure continued. A prospective study produced similar results. Controlling for severity of symptoms, Greenley (forthcoming) found that rehospitalization correlated negatively with the family's desire to have the patient at home. Thus both of these studies suggest that in middle-range cases, social factors like resources and family desires become crucial.

In assessing the cross-cultural evidence, Edgerton (1969) reached similar conclusions. He argued that markedly bizarre, chronic symptoms would probably result in recognition and labeling in any culture; however, most symptoms are not both chronic and extremely bizarre (Edgerton, 1969; Strauss et al., forthcoming). The majority of cases, therefore, are the subject of "negotiation"—i.e., factors like social status and resources, available roles, and family desires become important determinants of outcome. Edgerton's (1969) inferential analysis is thus perfectly consistent with Strauss and Carpenter's (1972) empirical findings. As noted above, they found that work, symptoms, social relations, and length of hospitalization interacted as open systems in determining outcome. The partial independence of these systems was illustrated by cases in which hospitalization as a measure of outcome was independent of the other systems because of the family's ability (or lack thereof) to care for the patient, the presence of job opportunities, or legal requirements. All of these variables, including hospitalization, could operate independently of the patient's clinical status. These findings directly mirror Edgerton's (1969) cross-cultural evidence and Miller's (1965, 1967) analysis of outcome.

Empirical investigations of status resources and type of commitment (voluntary vs. involuntary) are consistent with the preceding studies. Rushing and Esco (1977) found that marital status and resources interacted with severity of symptoms in determining type of commitment. For mild or moderate symptoms, marital resources correlated with type of commitment: The more marital resources, the more likely the commitment was voluntary. Marital resources, however, seemed to have little effect when deviant behaviors were extremely bizarre or disruptive. These commitments tended to be involuntary regardless of marital resources (1977): "When an individual's behavior is unquestionably bizarre or disruptive, advantages-disadvantages associated with varying degrees of marital resources do not modify the reaction of the community" (p. 142).

A subsequent study of the effects of education (as an index of socioeconomic status) on type of commitment produced similar findings. Rushing (1978) found that amount of education correlated with type of commitment: The more education, the more likely the commitment was voluntary. This relationship, however, did not hold true for the "severely impaired" category. Commitments for this group tended to be involuntary regardless of education status.

All of these studies suggest that a specific interactional perspective can parsimoniously encompass data from a variety of approaches

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and disciplines. For individuals with obvious, stereotyped, chronic symptomatology, the societal reaction is probably fairly predictable: The dangerous will be subdued and the ludicrous will be ridiculed (Murphy, 1976). The majority of cases, however, are much more ambiguous, and it is in these cases that social factors can have their greatest impact on outcome. Logically, these are the cases that best support a sociocultural approach to mental disorders. In contrast, symptoms that are severe, unambiguous, chronic, and that seem *relatively* immune to social influences best support a clinical-universalist approach (Price, 1973; Edgerton, 1966, 1969; Townsend, 1978).

The evidence we have examined suggests that knowledge of diagnostic reliability and validity is essential to an understanding of the effects of social factors on psychiatric practice. An ignorance of one implies an ignorance of the other. Unfortunately, many contemporary clinicians consider discussions of diagnostic reliability of purely academic interest, and there are movements afoot to discard psychiatric diagnoses entirely, or, at most, to take a cavalier attitude toward them (Kendell, forthcoming). This is a dangerous course for the following reasons: (1) Without reliable diagnostic categories, reliable prognosis and treatment are impossible; (2) a lack of interest in diagnoses obscures the fact that they can have tremendous impact on a patient's fate; (3) ignorance of diagnostic unreliability conceals the influence of social variables and thus inhibits objective study of the weighty moral, legal, and clinical implications of psychiatric treatment. The following questions therefore deserve careful, scientific investigation:

1. How many patients in ordinary clinical settings are reliably diagnosed?

2. Do psychiatrists in other countries agree on these diagnoses?

3. What are the recognition thresholds for various mental disorders in different countries among laypersons and among professionals? Can virtually anyone perceived as already labeled be diagnosed as psychotic?

4. How does diagnostic reliability relate to treatment response? Would some patients not reliably diagnosable (or those who are) do better without receiving psychiatric treatment? What do they need?

Solid empirical studies are beginning to answer these questions, but additional inquiry is needed.

NOTE

1. The model described here actually involves two types of interaction: (a) the statistical interaction of the determinants of outcome, e.g., work, social relations, symptoms, and length of hospitalization (Strauss and Carpenter, 1972); (b) the "social" interaction of the deviant with others. Significant indices of the latter are usually included in the determinants of outcome in major studies, e.g., in "symptoms," and "social relations" (Strauss and Carpenter, 1972; Miller, 1965, 1967).

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